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META-THEORETICAL PROBLEMS OF PSYCHOANALYTICAL INTERPRETATION OF LITERATURE

Norbert GROEBEN *

0. The problem in general

The psychoanalytical interpretation of literature is one of the few interpretive procedures within the domain of analysing literary texts that is grounded on an explicit theory, partially even on a theory of literary production and reception. Compared to some other procedures of literary interpretation (e.g. the 'immanent text' interpretation) this is its stronghold (cf. Groeben 1972: 132f.), but it is also its weak point: for all problems of the psychoanalytical theory (PAT) consequently weigh on the psychoanalytical interpretation of literature (PAIL), too. Concerning the meta-theoretical appraisal of psychoanalysis there is a long-standing controversy between psychoanalysts and the empirical analytical philosophers of science (cf. for a survey Möllenstedt 1976; Möller 1978; Perrez 1979). The empirical analytical camp does not stop stating and founding the 'teenager-thesis' in different versions, with different arguments, and with different emphasis and radicality: their thesis is that psychoanalysis is a science in the age of puberty, at least a science that has not yet reached the commonly accepted standards of scientific disciplines (cf. Demos 1959: 329). In general, the psychoanalytical party objects vehemently and with commitment that this problematized propagation of certain scientific standards absolutizes just one certain scientific tendency and that, in addition, that thesis more or less lacks the structure of PAT, i.e., that psychoanalysis is a science *sui generis*. If we now try to evaluate these meta-theoretical controversies as undogmatically as possible, we shall have to take into account at least these two aspects:

(1) that during the last 20 years many empirical analytical criteria of science have become problematic as to their precise, coherent explication, i.e., as yet, they do not exist as precisely formulated criteria but rather as, in part, incompletely explicated regulative ideas of goals (cf. Groeben and Westmeyer 1975: 228);

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(2) that, meanwhile, there are relatively elaborated designs for reconstructing psychoanalysis as a hermeneutical science (in contrast to the self-conception of orthodox psychoanalysis; cf. Lorenzer, Habermas, etc.). However, it is precisely this last aspect which, again, is liable to establish a dogmatic dichotomization which I think to be unnecessary.

In section 1, therefore, I shall attempt to touch upon and evaluate the most important points of the meta-theoretical criticism of PAT. I shall assume there that psychoanalysis is both willing and able to understand and to explain (cf. below, sections 1.2 and 1.5). Moreover, for an appraisal of such a criticism, I shall consider the partially insufficient state of explication in the classical criteria of science as they have been proposed by the empirical analytical philosophy of science, and, as a result, I shall, therefore, restrict myself to those necessities of improving the scientific structure of PAT, which, I think, are really imperative. In section 2, I shall then draw the consequences from the first section for PAIL, that is, I shall evaluate the hitherto existing perspectives of questions in PAIL from a meta-theoretical and methodological point of view. This will lead us to the diagnosis that, in the end it is first of all (or even only) the reception analytical approach by Holland (1975) that represents a methodologically consistent and meta-theoretically advanced version of PAIL.

1. Meta-theoretical problems in PAT

1.1. Problem: precision of terms

Considering the insufficient precision or vagueness of psychoanalytical terminology, we can distinguish between the following variants of criticism:

- the terms are not clearly defined, particularly in respect of the relation to other psychoanalytical terms (cf. Möller 1978: 57);
- some terms have different variants of meaning (e.g. the use of 'oral' on different levels of abstraction within the concepts of oral erotic, oral character, oral fantasies, oral dependence in the theory of identification processes; Möller 1978: 58);
- the terms are merely linked indirectly to observational data (in clinical observation as well) via other theoretical terms; this holds especially for the theoretical terms of metapsychology which, as a rule, are not directly 'operationalized' (cf. Möller 1978: 61ff.);
- especially, these meta-psychological, theoretical constructs are partially derived from highly different levels of language, sometimes in a completely mixed version, e.g. from: physiology, biology, action theory, etc. (cf. Peterfreund 1971: 84);
- quantitative factors are implicitly introduced into models in which quantitative functions are totally unknown (cf. Schalmey 1977: 137).

In an appraisal of these criteria, we should not forget that the relations between psychoanalytical concepts should, of course, be defined as explicitly as possible; generally, the use of single terms in different meaning variants, e.g. on different levels of abstraction, ought to be usually clear from the context, at least for the specialist. An explicit definition would certainly be helpful to the non-professional psychoanalyst, however. Yet, its absence does not necessarily disavow PAT as a scientific system of statements. Regarding the operationalization of theoretical terms, the meta-theoretical discussion and reconstruction during the last ten years, have shown, anyway, that in other scientific domains outside psychoanalysis, too, there is quite a number of constructs which are linked to observational 'data' only indirectly (furthermore, the concept of 'data' has become problematic, if not suspect, because of the dependence of observation on theories). Hence this phenomenon is certainly not appropriate to basically disqualify PAT against other theoretical designs: but a bloating of theoretical terms, which are eventually linked to 'experience' multi-indirectly, can have potentially immunizing effects. This aspect will be discussed in section 1.3. It is, of course, not optimal to refer to terms from different language games or model areas. Unfortunately, however, this happens even in the domain of the empirical analytical conception of psychology (cf. Herrmann 1982). The pretence of quantitative explications of functions might be a burden inherited from Freud's ambition to establish psychoanalysis as a natural science - a burden which PAT could possibly overcome on the basis of its present state of development, however.

1.2. Problem: explanation

Concerning the problem of 'explanation', 'explanation' is generally conceived of as the Hempel-Oppenheim schema of scientific explanation, including its meanwhile liberalized variants (cf. Groeben and Westmeyer 1975: 80f.; Perrez 1979; Möller 1978: 15ff.). Now there is a strange alliance: not only diehard 'neo-positivists' like Eysenck (1973), for example, maintain that, due to its fundamental non-scientificity, psychoanalysis is incapable of explanation, but the reconstructions of the meta-theoretical state of psychoanalysis by the proponents of the Frankfurt School amount to showing that PAT does not provide explanations, because it does not want to, since it is a hermeneutical science (e.g. Lorenzer 1972, 1974). Against those positions, I think, it can be stated on grounds of hitherto existing reconstructive attempts that, in principle, PAT does want to explain (cf. Mentzos 1973) and is also able to do so (cf. Perrez 1979). The reconstruction of the 'Wolfsmann neurosis' by Perrez, for example, clearly works out structures that are compatible with those of the liberalized version of the Hempel-Oppenheim schema. This is not even contradictory to the fact that certain postulates are not (yet) fully satisfied in that reconstruction: Perrez observes that, for example, the necessary generalizations

(general statements) often are not explicated, that their degree of confirmation is hardly demonstrated, etc. (1979: 99f.). Werthmann points out that the explanans has to be established primarily "with reference to the structuredness of mental events" (1982: 903). Möller diagnoses PAT to provide, above all, elliptic, partial explanations or explanatory sketches, that is to say, incomplete explanations (1978: 34.; here one could certainly add the variant called the 'How-could-it-happen-that-explanation': cf. Groeben and Westmeyer 1975: 96ff.). All these variants of incompleteness and imperfection are likely to occur in other partial domains of the empirical analytical conceptions of psychological theories as well. In evaluating this fact, it must furthermore be considered that for the social sciences, and their probabilistic hypotheses in particular, the philosophy of science, too, has not yet succeeded in sufficiently separating explanation from foundation (qua rational expectation of events) (cf. Lenk 1972: 53). Against this background, I think, it can be stated that PAT is obviously highly 'narrative'; yet, where its ability and attempts to explain are concerned, PAT is not, in principle, beyond the scope of the scientific standards common to social science.

1.3. Problem: hypotheses or tautologies?

It is more difficult to unravel the problem of the immunization of psychoanalytical statements against criticism or against the invalidation through experience. The oldest and most trivial variant of this immunization is the rejection of criticism on psychoanalysis, arguing (*ad hominem*) that such a criticism expresses a psychic resistance against PAT (an immunization quite common to Freud and his immediate successors; cf. Achten 1981: 20; Möller 1978: 83; Perrez 1979: 18) – today, however, this type of immunization is no longer propounded seriously. It is, therefore, more important and more decisive to look at those variants of immunization which critical rationalism has generally (i.e. not only with reference to PAT) criticized as the formulation of tautologies: hypotheses (in particular) are formulated in such a way that they apply to all possible empirical cases, and hence they can never be invalidated through reality or experience. So they are logically true, but, in turn, they have no empirical content at all. Möller puts forward the following five possibilities as virulent variants of immunization within PAT (1978: 76):

1. Formulation of tautologous statements
2. Imprecise terminology
3. Insufficient empirical interpretation of theoretical terms
4. Immunization through ad-hoc-hypotheses
5. Vicious circle between theory and experience."

Points 2 and 3 have been discussed above: we can talk about immunization in terms of tautology only in cases of really extreme variants of these possibilities. However, the other variants are indeed virulent for PAT: that is, up to now at

least, PAT is structurally in constant danger of applying these forms of tautology as an immunization against an invalidation through experience.

Examples

Möller quotes the following sentence as an example from the Wolfsmann account by Freud: "Conflicts between the ego and libido in early childhood lead to neuroses or not" (1978: 77).

Complex depletions of empirical content are certainly more common; for example, a large set of situational features in highly different combinations is postulated as antecedent conditions for certain events such that there is practically no chance of falsification anymore. Möller illustrates this by referring to the psychoanalytical postulates explaining enuresis (1978: 77).

In their immunizing aspects, those defence mechanisms are particularly dangerous which make it possible to harmonize any experience or any event with the psychoanalytical postulate: "Thus, a personality, for example, which is distinguished by its enormous generosity, can be conceived of as an 'anal character', although this personality is normally characterized by economy. In such a case, generosity can be interpreted as the formation of a reaction against the repressed tendency towards economy" (Möller 1978: 87).

Now, this is indeed a major handicap to the scientific status of PAT, regardless of which concept of experience one wants to apply, of what one wants to propagate as falsifying 'data', etc. (cf. below, sections 1.4 and 1.5). If, in advance, the postulates or hypotheses are emptied of all empirical content by immunization, it will be completely irrelevant in which manner one thinks of testing them (be it hermeneutical or empirical) – they can only turn out to be true, precisely because they do not tell us anything about reality. From the viewpoint of PAT, one could now of course object that the examples mentioned above (and those discussed in the literature) are false reconstructions, false explications or applications of psychoanalytical laws. Then, of course, it would not be legitimate to maintain that, concerning this immunization, PAT were in structural, latent danger. Nevertheless, I think that there is enough evidence to prove that, as a matter of fact, psychoanalysts are above all oriented towards demonstrating that, in principle, PAT can interpret or explain everything, that reality yields to its concepts, postulates, laws, etc. – even if immunization is a high price to pay for this orientation. I want to illustrate this issue by another example in which this orientation is clearly explicated by the psychoanalyst himself.

Specht (1981) explicates dream analysis "in its meta-theoretical aspects"; he begins with the wish fulfillment hypothesis, i.e., with the thesis "that all kinds of dreams are hallucinated situations of the gratification of desires" (Specht 1981: 765). For the amateur or the non-psychoanalytical psychologist this

thesis is highly implausible because it is intelligible by direct experience only for a small number of dreams. Specht even admits this and then explicitly puts forward a further differentiation of the hypothesis: "Dreams of hunger and thirst as well as certain simple dreams of wish fulfillment in children produce initial evidence for the acceptance of the wish fulfillment hypothesis. Relying on this evidence, the theory now proceeds in such a way that by gradually introducing new principles of interpretation, it is attempted to subsume under the principle of wish fulfillment all those dreams which, superficially regarded, do not display a tendency towards wish fulfillment" (1981: 766). Specht then takes the following steps in order to subsume more and more possible dreams under the principle of wish fulfillment: introduction of the censorship principle (o.c.: 766), the technique of free association (o.c.: 766), addition of further rules of interpretation concerning condensation and repression in particular (o.c.: 768), introduction of the dramatization principle (o.c.: 770), the principle of secondary elaboration, and the principle of the infantile as the source of dreams (l.c.). What remains, then, is nothing but traumatic dreams: as a possible final step, Specht refers to the interpretation of Schur, i.e., "traumatic dreams are conceived of as the expression of the desire for undoing the traumatic experience" (Specht 1981: 771) – Specht himself does not follow this interpretation. Nevertheless, the example – regardless of whether one takes that last step or not – shows how at first sight the wish fulfillment hypothesis grows "stronger and stronger" through additional hypotheses, by subsuming more and more possible dreams. *De facto*, however, it grows weaker and weaker because, in the end, it can no longer be tested against reality. It seems to me that the central danger to psychoanalysis as a total model is indeed this form of 'being right' in PAT through a partially even circular immunization *qua* empirical depletion of hypotheses or laws.

1.4. Problem: empirical confirmation?

The problem of testing and confirming psychoanalytical hypotheses empirically, shows above all how, in part, the discussion between the empirical analytical camp and the psychoanalytical position has got stuck and blundered: the empirical analytical philosophers of science generally adhere to the view that PAT must be tested outside the psychoanalytical (therapeutic) situation (be it by experiments, quasi-experiments, field or correlation studies, etc.; cf. Perrez 1979: 17; Möller 1978: 96ff.). The psychoanalysts, however, object that this view involves a concept of experience which is totally inadequate to psychoanalysis, that is, psychoanalytical hypotheses can be tested only within the psychoanalytical situation itself, within the framework of clinical methods, and hence only on the basis of clinical data. This line of argument then brings on the counter-argument of the empirical analytical side, i.e., that "in this way one can merely confirm assumptions about the effectiveness of the therapy"

(Möller 1978: 95; cf. also Achten 1981: 24). Against this skeptical view one can put forward (as, e.g., Mitscherlich and Rosenkötter do, 1982: 1152f.) that the critical psychological research on methods has demonstrated that a partially uncontrolled but in any case implicit set of artifacts manifests itself even in quasi-experiments and experiments, e.g., via experimenter effects (Rosenthal 1966). Whoever gets tired of all that toing and froing in arguments about methods, is easily tempted to come to a decision on those empirical experimental tests of psychoanalytical hypotheses and their confirmation, which are so far available. Even here he is not given final security: Kline (1981: 433ff.), for example, finds relatively much empirical confirmation for the tested parts of PAT (especially for its lower levels of abstraction); quite apart from the fact that the meta-psychological concepts, in particular, have not been tested and partially are not testable according to Kline, the confirming results, too, may be explained in part just as well or even better by models of learning theory (o.c.: 441ff.) Moreover, Sears (1952) arrives "at an essentially worse evaluation of evidences for the psychoanalytical theory" (Möller 1978: 102) by looking at mostly the same investigations.

If one is acquainted with the common degree of confirmation for psychological theories, including the problems of comparing investigations, of methodical neatness, of statistical significance *vs.* variance explication, etc., then the empirical, experimental results, available so far, will at least not appear to be clearly disqualifying for PAT. In the end, however, one must again enter the methodological level of argumentation: and here, despite all patience with the psychoanalytical straining of a non-experimental concept of 'experience', I do not see why the clinical data from therapy, for example, should not be secured and worked up more intersubjectively (e.g. by psychoanalytic clinicians). That is, also in regard to psychoanalysis, psychoanalytical therapies and to other branches of applied psychological research in therapy, it seems to me quite legitimate to demand that psychoanalytical interviews be recorded or filmed, that the therapist's behaviour must be analysed during and after the psychoanalytical session, etc. (cf. Shakow 1960 according to Perrez 1979: 157) – for example, within the framework of research via supervision as is usual in all other psychological schools of therapy.

1.5. Problem: intersubjective communication

The point where the potentially autonomous concept of experience in psychoanalysis becomes virulent, is the level of the so-called basic statements: i.e., those statements, in which reality *via* experience is supported to depict itself as directly as possible. In the empirical analytical conception of science this problem is solved, first of all, by taking recourse to perception, irrespective of the problems of explicating an observation language or an empiricist basic language. Recently, however, it has been objected that the 'basis' of the clinical

method is not so much constituted by observation as by understanding (e.g. Ricoeur, Habermas, Lorenzer, etc.; cf. Achten 1981: 28; Möller 1978: 120ff.). Nevertheless, it is just this aspect of understanding which is (co-)responsible for the classical criticism by the empirical analytical philosophers, i.e., that the clinical 'data' could be the result of the therapist's suggestion, and, further, that rivaling schools of psychoanalysis confirm their theories by means of the same clinical methods (cf. Perrez 1979: 23).

To counter this criticism the psychoanalytical side can refer to the following control factors: "The 'freely floating attention' of the analyst, the control of counter-transference, the preservation of the 'analytical incognito', and ... the isolation of the patient and the analyst from outside influences ..." as well as "the large quantity of data ..., that results from the length of a therapy ..." (Möllenstedt 1976: 69). The main criterion, however, has already been established by Freud, i.e., the patient's agreement on the analyst's interpretation. Here he distinguishes direct from indirect confirmation, assigning more importance to the latter (cf. Möller 1978: 125). There is indirect confirmation, "if the patient, immediately after he has said 'yes!', produces new memories which complement and enlarge the construction" (Freud according to Perrez 1979: 129f.). Of course, for the empirical analytical philosopher of science it is, above all, the construct of the patient's resistance that remains suspect; following Freud, one could postulate here that the patient would first of all react to a misinterpretation through indifference (cf. Perrez 1979: 129). It is particularly this aspect of the patient's agreement on the analyst's interpretation that has been reconstructed by members of the Frankfurt School within the conception of the consensus-theoretical criterion of truth (cf. Habermas 1968; Apel 1964/65). On grounds of these reconstructions we can now locate three appraisals of the problem of intersubjective communication:

(1) only the therapist's experience of evidence (in the frameworks of monological understanding) is not sufficient as a criterion for deciding the correctness of an interpretation (cf. Möller 1978: 121);

(2) concerning first and foremost the problem of the analyst's influence upon the patient, it is reasonable and necessary to provide testability open to other researchers (see above and Möllenstedt 1976: 99), i.e., "that other researchers get direct access to the observable phenomena of the psychoanalytical process" or "to make accessible to other researchers the observable phenomena of the psychoanalytical two-person situation through sound film control" (Möller 1978: 126, 129);

(3) the consensus-theoretical reconstruction of psychoanalysis by Habermas, Apel, and Lorenzer as dialogical understanding implies the postulate of a counter-factual idea of optimal discourse situations; but even this counter-factual idea of a dialogue without power can be explicated and empirically tested according to concrete framework conditions (cf. Groeben and Scheele 1977: 176ff.).

1.6. Results

Concerning the meta-theoretical problems of PAT, we can now draw the following conclusions.

Where the precision of concepts, the problem of explanation, and the empirical confirmation of hypotheses are concerned, psychoanalysis in fact shows many problematic aspects. Yet, these do not justify stating a structural difference between psychoanalysis and other social theories in psychology; rather, one may regard these aspects as the price to be paid for the heuristic power of psychoanalysis. By postulating new, unexpected hypotheses, violating everyday plausibilities, psychoanalysis surely has that power (cf. Möllenstedt 1976: 109f.). Concerning the basic problem, however, it must be observed that the clinical method of psychoanalysis cannot be reconstructed and construed as monological understanding but only as dialogical understanding. Here, the biggest problem clearly lies in the formulation of psychoanalytical hypotheses: and this is where PAT is structurally in danger of striving for verifications through immunization *qua* the formulation of tautologies (i.e. depletion of empirical content). A good deal of rethinking is really needed on this problem in PAT: its critics would doubtless be more satisfied, if PAT faced and answered the following two questions:

- what are the points, the domains, and the conditions that make a psychoanalytical interpretation inadequate or unreasonable?
- on which points under what conditions, and with what criteria can a psychoanalytical interpretation be proved to be false?

A final example, which is directly relevant to PAIL because of the parallels between dream analysis and literary interpretation, shall now illustrate these points: concerning the following question one may follow Eysenck, whose reduced neo-positivist position should otherwise be rejected in general: "How can a dreamer dream of something that has *no* reference to sexuality?" (Möller 1978: 79). In a thought experiment, Eysenck mentions the example of a journey by train in which one will discover a wealth of sexual symbols, if one looks for them adequately (Eysenck 1957/58: 174f.; cf. Möller 1978: 79f.). Looking at the history of PAIL, we know that, unfortunately, the pan-sexualist interpretation indicated by Eysenck is not a caricature (cf. Groeben 1972: 128ff.).

2. Consequences for PAIL

2.1. Rejecting the classical exopoetic and endopoetic PAIL-approaches

Against the background of the meta-theoretical discussion on PAIL, it is quite clear that, as yet, PAIL has not made extensive use of the constructive

possibility of a dialogical hermeneutics based on the method of free association, and in fact, PAIL has even failed to do so. Hitherto, PAIL has reduced itself to a monological hermeneutics, that is, it has not realized consequently the methodologically basic approach of collecting free associations, of the agreement of a "living consciousness" (Edel 1951) on the interpreter's interpretive proposals. So, even the free associations of the reading interpreter have, as a rule, not explicitly been included into the representation of literary interpretation. Worse even, the aspects of transference or counter-transference have not been thematized, as Goeppert and Goeppert (1981: 88ff.) postulate. Thus, both the exopoetic and the endopoetic variants (Eisler according to Dettmering 1974: 19) of classical PAIL are found to be insufficient. The exopoetic variant is based on the functional analogy between production and product (cf. Groeben 1972: 96), i.e., one arrives at the producer (namely, the author), by inferences from the expressive meaning form of the product. This is the classical variant of PAIL: namely, the psychoanalytical biography of individual authors *via* their literary texts (cf. also Perrez 1979: 183ff.) The above sketched meta-theoretical analysis clearly displays that the very basic conception of a psychoanalytical interpretation oriented towards the author would only be admissible as a methodologically coherent approach, if the author himself were involved in a clinical, dialogical situation as a 'living consciousness' with free associations. In practice, however, this does not hold for all those cases of psychoanalytical biography which I know of. There are even more arguments against the classical variant, such as: how does the analyst get to know from what literary figures and their features the author can be inferred? Such arguments lead to the conclusion drawn pointblank by Wünsch: "Hitherto, inferences about the author are, at least methodically, completely illegitimate" (1977: 54).

I am afraid that almost the same argument applies to the endopoetic variant of PAIL, too. This variant is based on the qualitative analogies between general features of the product and its content (cf. Groeben 1972: 97): i.e., the characters described in a literary text are interpreted by the models of depth psychology (cf. also Perrez 1979: 183ff.). This approach of a text-centered, hermeneutical PAIL, too, falls short of the meta-theoretical possibilities and methodological necessities of PAT. Wünsch (1977: 51) is quite right to refer to the fact that the psychoanalytical interpreter has to manage with data "which are selected according to principles different from that one of whether these data are relevant to the psa systems of classification or not", i.e., that actually PAIL is provided with only a part of those data which are basically relevant in psychoanalysis. Furthermore, methodically it is not settled at all what data have to be taken into account for literary interpretation. Following Wünsch, the practice of immanent-text PAIL apparently follows the rule: "Anything *may* but *nothing must* be taken into account" (1977: 52). This arbitrariness in the selection of data disqualifies this variant of PAIL as an insufficient approach,

too – totally independent of meta-theoretical standpoints. It is, therefore, not surprising at all that, up to now, immanent-text PAIL is still characterized above all by schematism and reductionism: "So, apart from just a few exceptions, the psychoanalytical criticism of literature practised hitherto is restricted to the reductionism of 'latent content', of 'unconscious motivation', of 'hidden patterns' and 'unconscious meaning'" (Achten 1981: 151).

2.2. *PAIL as reception research (Holland)*

In contrast to the reductionism of monological hermeneutics, Lesser (1960) and Holland (1968), in particular, have devised a psychoanalytical theory of literary reception which is based on the idea of active reception of (response to) literary works. On the basis of such an active reception, construed as a kind of creatively producing literary texts further ('analogizing' in Lesser's terminology), the 'living consciousness' thus becomes far more important than before. For, the processes of constituting the meaning of symbols (fantasizing, defense mechanisms, etc.), as Holland (1975: xiii) puts it, do not take place in books but in the reader's consciousness:

"Processes like the transformation of fantasy materials through defenses and adaptations take place in people, not in texts" (Holland 1975: 19).

This orientation leads to relativizing the interpreter's position: the response of the scientific interpreter is just one among many possible responses, and not to be identified with 'the text itself'. Holland ironically asks:

"Otherwise, if interpretation flowed structurally or necessarily from the text, why would critics sign their work?" (o.c.: 40).

So, the psychoanalytical theory of literary reception catches up with the functional concept of text in modern hermeneutical and empirical reception aesthetics through the consequent elaboration of its methodological basis: in the postulate of the reader's meaning-constitutive function (cf. Groeben 1982; Schmidt 1982). In this basic work (1975), Holland again and again formulates this constitutive function of the reader for investigations into meaning, for example in the following principle:

"... that the fantasy is not 'in' the work but in the reader or, still more accurately, in the creative relation between reader and work" (o.c.: 117).

On the basis of this literary, aesthetic position, he is indeed willing to draw the consequences for the conception of literary research, i.e., consequences which – in the domain of discussions on the theory of literature – have not been drawn yet by reception aesthetics, although they have been propagated in the empirical conception of literary studies (cf. Groeben 1980, 1981; Schmidt 1975, 1980):

i.e., that an academic interpretation of literary texts can no longer be focussed on *the* literary text itself, which represents nothing but the interpreter's subjective reception anyhow, but has to account for the processes of reception and its content. Holland fully utilizes the dialogue-hermeneutical potential of psychoanalysis and thus performs the paradigm-change towards research in literary reception as research in the literary work (analysing the work *via* the reader).

Norman Holland's "5 Readers Reading" (1975) is the book in which this psychoanalytical conception of reception research is paradigmatically elaborated and presented. As a result of the above sketched dialogical orientation, his central methodical approach consists in collecting the free associations of readers about a given literary text within an unstructured interview (o.c.: 44). As such an analysis of literary reception with maximum freedom in respect to the (associational) responses of individual readers would require large-scale case studies, Holland examined only the reception of five readers. Alluding to the abbreviation for test subjects (Ss), he gave his readers fictitious names beginning with 'S': Sam, Saul, Shep, Sebastian, and Sandra. Despite his dialogue-hermeneutical, 'soft' method, Holland attempts to realize a theory-guided strategy of research: for this purpose, he starts his investigation with the general hypothesis that the reader's active, productive constitution of the meaning of the literary text represents the production of sense within the frameworks of the reader's individual answers and questions about sense in respect to his 'self' and his environment. In order to explicate such a focussing of sense in a central psychoanalytical concept, Holland then develops the construct of the 'identity theme' (o.c.: 53ff.). The interpretation of the collected (associational) data of reception consists in connecting the individual reader's concrete literary reception with the reader's individual identity theme. In doing so, the interpretation is directed towards working out general principles of literary reception by generalizing individual case studies. In order to avoid circularities in connecting reception data with identity themes, Holland bases his explication of identity themes not (only) on the readers' associations about the given text, but also (above all else) on the results of established projective tests like the Rorschach test, the thematic apperception test (TAT) and the so-called COPE questionnaire for determining defense mechanisms (o.c.: 52). Holland confronted his readers with short stories because he expected to get simple, straightforward associations and comments most easily in that way. The short story on which most reception data and interpretations were passed, was "A Rose for Emily" (by Faulkner).

The psychoanalytical reception research does not only allow to link PAIL with empirical, social analyses of literary processes, as I mentioned above, but this type of research, I think, also represents the only coherent version of the psychoanalytical methods of interpretation. At present, I think, there is no other meta-theoretically constructive and productive possibility for PAIL than to pursue and elaborate Holland's approach. Its further elaboration – especially on the basis of non-circular testing through the integration of e.g. other modes of inquiry, as Holland already demonstrated by means of projective tests – will then be capable of answering the central, confidence inspiring questions within the domain of PAIL: i.e. for what object domains (text types, textual meaning, text themes, etc.) PAIL is not reasonable, inadequate, or incompetent, and how errors in the psychoanalytical interpretation of literary reception processes and textual content can be detected.

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EXPANDING THE METHOD

Comments on the papers by Orlando, Sullivan and Groeben, and the potential for psychoanalytic criticism

Joseph WESTLUND *

In addition to commenting on the articles, this essay points to some largely unexplored approaches – Klein's reparation, newly awakened interest in narcissism – and, more broadly, discusses the problems inherent in the very idea of 'methodology'.

First, I shall comment on the papers, and then on the wider possibilities open to psychoanalytic interpretation: particularly the social, readerly implications.

The methodology of psychoanalysis – and of literary criticism – can never be as precise as many would like it to be. Initially, Freud hoped "to furnish a psychology that shall be a natural science" ("Project for a Scientific Psychology" (1950[1895])). His view radically shifted as he explored the unconscious, although he remained ambivalent – as in his fondness for a quantitative approach to psychic energy. Freud never published the "Project"; indeed, when in his old age he was presented with it afresh, he did his best to destroy it (p. 290). James Strachey invented the title, and although it captures the tone of the work itself, it fails to reflect what psychoanalysis had become over the years. When Marie Bonaparte, Anna Freud and Ernst Kris first published the work, they chose a title which far more accurately captures their view of its importance in the discipline: "Entwurf einer Psychologie," or "Sketch of a Psychology." Because of the "Project" and Freud's attraction to natural science, many have been misled into emphasizing the 'scientific' nature of the discipline and worked themselves into a dead end. A recent book by three psychoanalysts, for instance, begins with the statement that "because of the inadequacy of psychoanalysis both as a field of inquiry and as a therapeutic institution, it has lost prestige in North American culture. For the most part it is seen as passé ... an unrewarding withdrawal from mundane concerns into a rarefied pursuit of truths which lie buried in the deepest layers of subjectivity" (Gear et al. 1981: 4). Such a view pretends to an overly-exact and exclusively-medical technique. Freud conceived of psychoanalysis as a mode of inquiry

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